JACC: CARDIOVASCULAR INTERVENTIONS
VOL. 8, NO. 8, 2015

ISSN 1936-8798/$36.00
PUBLISHED BY ELSEVIER INC.
http://dx.doi.org/10.1016/j.jcin.2015.06.005

EDITOR'S PAGE


Lost in Translation

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Robert J. Lederman, MD*

Ideas are important. Without ideas, nothing can happen. However, most ideas die before anything useful develops from them. Interventional cardiology was born from ideas and prospers from ideas that are translated into useful medical innovations. I was reminded about this gulf between basic ideas and useful discovery when I heard of the death of my friend, Jack Vogel after a long illness. Jack was addicted to new ideas and always tried to promote them at his Snowmass course and at the Santa Barbara courses specifically designed to bring the latest concepts in interventional cardiology. In the late 1980s, a time we called the new device era, every innovator was invited to those Santa Barbara meetings and a similar one put on by Michel Bertrand in Monaco. We saw all kinds of devices including directional atherectomy, extraction atherectomy, rotational atherectomy, porous drug-eluting balloons, brachytherapy devices, multiple laser approaches including selective lasers to identify plaque and to ablate only abnormal tissues, balloon lasers, microwave angioplasty, spark erosion, early stents, and aortic balloon valvuloplasty. These were a few of the device ideas put forth in these conferences and in a book edited by Jack and myself (1). Many of these ideas died and appropriately so, but others took decades to reach clinical application. The bioabsorbable stent, an idea first suggested to me in 1986 by Jack Whitehead, founder of the institute at the Massachusetts Institute of Technology bearing his name, has now, almost 30 years later, gained the status of clinical application and critical examination. This wide chasm between ideas and clinical application is the domain of translational research. About a year ago, I received a letter from Dr. Robert Lederman of the National Heart, Lung, and Blood Institute (NHLBI). This is what he said:

> In April 2013, JACC published a letter describing the transcaval approach for TAVR [transcatheter aortic valve replacement] that NHLBI conceived and tested in animals (2). That publication connected Henry Ford investigators to NHLBI and led to clinical translation of a promising new method (3). Unfortunately, journal space appears to be declining for the animal feasibility experiments required to develop such novel treatments, and editorial boards increasingly downplay pre-clinical interventional work as "science fair" novelties not immediately applicable to clinical practice. I believe otherwise.
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> First, every talented physician has creative ideas but perhaps not the right opportunity to develop them. Pre-clinical "idea papers" inspire readers and sometimes connect them directly with authors, as in the transcaval experience. New concepts inspire clinicians to try new approaches, engineers to invent new devices, and industry to develop new products.
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> Second, trainees need to see that it is possible to contribute wholly new and immature concepts in interventional cardiology beyond testing others' commercial products, and without genuflecting to commercial interests.
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> Third, translation from concept to clinical medical device is arduous. Obstacles, whether financial, academic, or regulatory, are numerous. Translating pre-clinical feasibility into patients requires funding, and publication is a key step towards obtaining funding. Indeed selection for publication is akin to triage for funding.

From the *National Heart, Lung, and Blood Institute, Bethesda, Maryland.

Herein lies the problem. When publication thresholds are so high, important work gets missed. NHLBI's Michael Lauer recently reported the astonishing observation that the most highly ranked investigator-initiated NHLBI grant applications do not necessarily generate the most highly cited publications (4). This suggests that as selection criteria become too restrictive, they lose discriminative value.

Perhaps *JACC* can offer a solution, such as an online-only "early innovation" section. Such a feature would promote translational science and help bridge the funding gaps that separate proof-of-concept from first-human-testing of new treatments.

Despite tight space and tight budgets, it is critical to make new space for innovative pre-clinical cardiovascular "idea papers."

Well, Robert, the *JACC* family of journals is now going to address this issue. A new journal dedicated to translational research will provide a home for some of these pre-clinical ideas and discoveries. *JACC: Basic Translational Research* will be an open-access journal that is online only and will provide a venue for many papers now judged to be too early for *JACC* or the other members of the family, such as *JACC: Cardiovascular Interventions*. Obviously the submissions sought will cover the entire spectrum of cardiovascular medicine including drug therapy, cell therapy, genomics, and so on. Of special interest to us, however, will be the device and technique papers that we have not had space for in the past. Some have asked whether *JACC* and the other *JACC* journals will continue to publish translational papers, and the answer is yes. But, the new journal will provide an opportunity to bring many more new ideas with translational activity or potential to the clinical, as well as the scientific, communities. This publication is one aspect of the needed empowerment of translational research, and I will discuss another in a subsequent letter, but hopefully this will be a constructive step to reduce what has, in the past, been "lost in translation."

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